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WEAK CONTINGENCIES, STRONG CONTINGENCIES, AND MANY BEHAVIORS TO CHANGE

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It certainly is time for applied behavior analysis to go beyond its present parameters. In fact, it has always been time to do that; probably, it always will be. Kunkel suggests that now is a particularly appropriate time to remind ourselves of that, partly because it is the 20th anniversary of the journal that is still the hallmark of the field, partly because there does seem to be a fairly widespread evaluation within the discipline that it has somehow slowed, and partly because of a comparison of the first issue of the first volume of that journal with the first issue of its 20th volume. The first two of these three reasons probably need no debate; the third of course needs a larger and more representative sample and a searching assessment of rater reliability and social validity, but in my opinion, we should take it seriously even without that.

First, though, we should note that a journal prints only the best of what is submitted for review, even when the editor asks for better than that as forcefully as seems possible. If in our journal even those best studies seem often to be, as Kunkel indicates, "the endlessly repeated use of effective techniques to modify activities of individual children and patients," we may well worry—but only after we celebrate. Is there an objective, scientific, properly restrained way to report how literally beautiful and wonderful it is that more and more children and patients are endlessly but actually getting more and more of their problems solved, effectively and quickly? Before we find that alarming, or boring, we should find it immensely and durably valuable. If the solutions seem endless, perhaps we should note that the problems are endless, and that we do not complain that the medical doctors are solving more and more of our endless health problems. Like the medical doctors, those of us engaged in that endless business of quickly and effectively solving more and more problems of children and patients actually profit from reading those endless recipes: As in cooking, not every new recipe is self-evident, yet every new recipe enriches at least someone's life over baseline—even the lives of the physical chemists, who are the basic-science theorists of cooking, but who long ago became bored with its potential for contributing to their theory. In the vast ocean of inapplicable behavioral journals, surely there is room for a few that are applicable, even if their applications seem repetitive to readers who, so to speak, do not cook. There are, after all, no readers who do not eat.

Proceeding properly from wonder and gratitude rather than dissatisfaction, then, we may still ask, as Kunkel does, what is hindering us from solving the bigger problems that remain? I suggest that the answer is correlated with but not identical to the brevity of our studies and their frequent location in institutions, which Kunkel cites as the central problems. Those are merely marker variables; like age and social class, they correlate with many problems but neither explain them nor guide their remediation.

The central barriers, I suggest, are three-fold: (a) we are not empowered to try to solve those bigger remaining problems, (b) we have not yet made the analysis of how to empower ourselves to try them, and (c) we have not yet made the system-analytic task analyses that will prove crucial to solving those problems when we do empower ourselves sufficiently to try them.

Of these barriers, the first needs little or no study; it seems obvious. The second and third are the kinds of problems that seem analytic of the first, in that they themselves are potentially remediable,

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so the proper recommendation is of course that we turn to their solution with as much vigor as possible. Unfortunately, that recommendation, upon analysis, immediately turns up a Hellerian (1955) paradox: We are not empowered to find out how to empower ourselves to solve the bigger problems that remain, and thereby we are not empowered to conduct the research necessary for the system-analytic task analyses crucial to their solutions.

The solution to *that* paradox is to notice that it is based on an all-or-none dichotomy of empowerment, and to remember that life is rarely like that. It *is* self-evident that there are only two kinds of people in the world: those who think that there are only two kinds of people in the world, and those who don't. That dichotomy teaches us to join the second group—not to assume that "We" are unempowered and "They" are empowered, but instead to assume that a highly variable "We" are merely temporarily and quite variably underempowered. The questions for those whose environments make them want to is how to move along that continuum toward empowerment and what to do with the power increments that result.

A common tactic for the underempowered is to do both a little at a time. After all, the relevant behaviors are complex and poorly specified; they are often referred to unanalytically as social skills: fading in, joining, wangling, finagling, looking for severe but soluble crises rather than trying to lead people who do not want to be led, avoiding arrogance despite its reinforcement schedule, and not talking like a behavior analyst to people who think that they have free will, for a start. The point is that a system of quite powerful social and physical contingencies already exists in such problems, and that anyone trying to enter and alter that system with relatively weak contingencies will have to do so with an understanding that it is almost impossible, but only almost.

In my opinion, that is roughly what some researcher-practitioners are doing (see Gerald Patterson's entire professional output in the world of family therapy for the last two decades). Whether they are a "We" in contrast to a "They" is probably a counterproductive issue, much less important than that it is possible to behave that way (more or less). That few people do is quite understandable; there is still no objective public analysis of exactly what they are doing, and so it is not completely obvious either how to imitate them or how to do better. One-issue samples of one current journal may not even detect the presence of that kind of behavior, and even many-issue random samples of all relevant journals will show quite accurately that although that behavior exists, it is rare, as Kunkel argues.

If the behavioral analysis of the necessary empowerment is a class called social skills (for lack of a better analysis), then the problem is to analyze *those* social skills. Kunkel's brief summary of the Vicos project should sketch for us how those anthropologists managed to enter a system that probably was closed to any but a very small set of just-right entry attempts. That it took a long time seems quite likely, but is not the essential point. Probably that long time was filled with a series of behavioral interactions between enterers and residents. What was long may have been the necessary series of behaviors to be changed by everyone involved; each of those changes, I suspect, was or could be quite short, especially if there was an interim reinforcer for each change. If only a social-skill–mediated promise of some eventual natural reinforcement is available (e.g., full profit from a good harvest) then of course progress will be slow in calendar terms, if it occurs at all. Harvests, after all, come not very often—in some places, only once a year.

In my experience, those projects that seem arduously long are arduous because (a) I do not have a strong interim reinforcer compared to those in the existing system for status quo and must wait for opportunities when weak control may operate even so, or (b) I do not yet have a correct task analysis of the problem and must struggle through trials and errors. By contrast, (c) when I have an effective interim reinforcer, and I know the correct task analysis of this problem, long problems are simply those in which the task analysis requires a series of many behavior changes, perhaps in many people, and although each of them is relatively easy

and quick, the series of them requires not so much effort as time, and so is not arduous but merely tedious.

The remaining unsolved problems remain unsolved because of those three cases, I suggest—especially the first two, and not for any lack of correct behavioral principle. What is lacking, then, is a better analysis of the social skills that allow fading into existing systems, joining them, seeing opportunities in them, wangling, and so on.

Thus, Kunkel's argument may not be immediately actionable as it stands, but it is exceptionally valuable to this discipline nonetheless, because it lets us remind ourselves of what remains undone and that doing requires actionable hypotheses. Given that context, we can see that we shall not have the necessary empowerment for action of that sort bestowed on us, but must instead go out and program it. Then we can see that sometimes we are already doing that, but we also can see that we are not doing it nearly as often, as intensively, or as systematically as we could. Thus, the proper editorial review is, Thank you.

REFERENCE

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